to advertisements have also their interesting features, and the numerous illustrations and process plates scattered here and there add an additional attraction to the volume. The success of this present edition will be gathered from the fact that it has already been sold right out, as is stated by the *British Journal of Photography.*

Encyclopédie Scientifique des Aide-Mémoire. Le Vin. Par Henri Astruc. Pp. 208. (Paris: Gauthier-Villars, 1901.) Price 3·0 F.

THIS little treatise on wine-making is essentially encyclopedic in character, and as such calls for only a brief notice. The author is evidently familiar with his subject, and in the limited space at his disposal has been very successful in reviewing both the scientific and economic position of the French wine industry. There is nothing novel in the scientific questions discussed in this book, but some of the economic questions brought forward are not generally recognised in this country. For instance, here we have been inclined to regard wine growing in France as only in process of recovery from the devastation wrought by phylloxera, and it comes as a surprise to be told by the author that the wine-growers of his country are at present suffering from the effects of over-production.

This little book will be useful to anyone who desires to make a rapid survey of the present position of the French wine industry. A. J. B.

A Commercial Geography of Foreign Nations. By F. C. Boon, B.A. Pp. viii + 174. (London: Methuen & Co). Price 2*s.*

THIS book will not assist to make commercial geography a scientific study. Like the geographical books of old time, the volume consists largely of disconnected details which no pupil ought to be asked to remember, and which produce weariness of the flesh in the unfortunate reader. If commercial geography means what Mr. Bovn makes it, then it is the duty of all who are anxious for the introduction of reasonable methods of instruction in schools to condemn it at every opportunity. Here are a few examples of unqualified or loose statements which occur in the early pages of the book. "The greatest heat for the greatest number of days is on the Equator" (p. 1). "As the Equator is neared [from the Tropics] two days have vertical sunshine at each point within the Tropics, approaching gradually to the autumn and vernal equinoxes at the Equator" (p. 1). "Added to the effects of the neighbouring land or water are the similar effects of the winds that blow over them" (p. 2). "The Gulf Stream washes the coast of Norway" (p. 11). But we do not object so much to statements of this kind as to the principle of cramming pupils with information which has to be accepted without inquiry and cannot be assimilated. The less we have of commercial geography of this kind the more likely are we to create an interest in the study of the subject.

Mining Calculations. By T. A. O'Donahue. Pp. viii + 211. (London: Crosby Lockwood and Son, 1901.) Price 3*s.* 6*d.*

THE primary object of this little book is to enable candidates for certificates as colliery managers to obtain with a minimum of trouble a sufficient knowledge of arithmetic and mensuration to pass their statutory examinations. If the student will steadily work out the numerous useful examples given by the author, his chances of success will certainly be increased. Some of the absurdly easy arithmetical questions, quoted from the official examination papers, do not tend to enhance one's respect for the statutory certificate; however, this is no fault of the author, who has simply written a book to supply a want created by the examiners appointed under the Coal Mines Regulation Act.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonvmous communications.*]

The Inheritance of Mental Characters.

ALL biologists must be grateful to Prof. Karl Pearson for his extremely valuable and interesting paper reported in your issue of December 5, 1901 (p. 118). Inasmuch, however, as his conclusions are likely to be taken as the settled results of scientific research, it may be appropriate at this time to express certain doubts which naturally arise on reading the abstract. A man at the age of thirty, for example, possesses certain physical, intellectual and moral qualities. These must be due to more than one class of factors, and may possibly be due to three:—

(1) Heredity; characters derived from ancestors, including for present purposes the results of normal variation.

(2) Environment.

(3) Soul; supposing that the man is something more than an intelligent mechanism, and considering the possibility that his soul may have preexisted his advent here as an individual of *Homo sapiens.*

The third factor will be ridiculed by many, but if it has any reality it may eventually be capable of demonstration by just such methods as Prof. Pearson employs. The first and second factors are universally recognised.

Now it is apparent at once that the influence of the several factors is not the same on all the qualities of the man. Stature will depend almost wholly on the first set of factors, eye-colour wholly so. On the other hand, health will certainly depend largely on the second, so will shyness, intelligence, &c.

If, therefore, it is found that stature and eye-colour exhibit exactly (or almost exactly) the same degree of divergence from parental or fraternal standards as do health, shyness, &c., may it not be that this disproves just what it seems to prove, because A does not equal B, but equals $B+x$?

It may be said that the statistics given are based on pairs of brothers, whose environment must have been almost identical, and hence the second factor would not affect the divergence between them. But this appears a doubtful argument, because (1) the treatment of successive children is very commonly not the same, and the fact of being an elder child is itself influential; (2) germinal selection must be supposed to be going on from the earliest moment of existence, and very slight environmental factors may make great ultimate differences.

There is another consideration, that of the stability of the different qualities in the race. Characters which were highly variable would not appear to be inherited to the same degree as those which were very stable. This might also appear in cases of atavism, where the pendulum of variability took an exceptionally long swing, going back to ancestral features of which we possessed no record. Thus let the inheritance be expressed by A B C D E A B C D E A B, &c., instead of A B A B A B, &c. In the former case our data might only cover A B C D, in the latter A B A B. We should say that the individuals of the latter series came very "true," those of the former not at all, though the result in the long run might be about the same in either case. Lest it be said that the former series is wholly imaginary, I will cite the case of the domestic dog. The ordinary mongrel street-dogs in a single town would afford material for several genera and very numerous species, judging them by the physical standards we employ for wild animals. Yet the domestic dog, taken as a whole, has not changed very much in long periods. That is to say, the extraordinary variability presented is not progressive under existing conditions, and we return sooner or later to about the starting point. T. D. A. COCKERELL.

East Las Vegas, New Mexico, U.S.A., December 20, 1901.

I AM not unmindful of the possible influence of environment in increasing the correlation of brothers. I strongly suspect that home influences have a good deal to do with the rather exaggerated value for the fraternal correlation in the category of *conscientiousness.* But certain characters, *e.g.* the cephalic index after three years of age, the eye-colour between twenty and thirty

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and the hair-colour from seven to fourteen are but little, in England at any rate, subject to environmental influence. The cephalic index remains almost constant throughout life. Now the mean value for the fraternal correlation for these three characters is 0·5161, and for stature, span, forearm and health, which might be supposed to be largely influenced by environment, it is 0·5179. It thus seemed to me that environment was not an important factor in modifying the correlation of the *physical* characters between brothers. In other words, *environment does not influence the constant uniformly in one direction.* This view was apparently appreciated by Prof. Cockerell when he wrote "the treatment of successive children is not the same." Any influence of environment, strange as it may seem, was thus found to be negligible. Turning to the intellectual characters, my own *a priori* conception was that I should for the first time be able to distinguish between nurture and heredity. I expected much higher correlations in the case of temperament and probity than in that of physique. I was therefore somewhat surprised when the values came out much the same as in the case of the physical characters, say an average of 0·5. Of course it is open to Prof. Cockerell to say that 0·3 of this only is due to heredity and 0·2 to environment, or whatever other division appears to him probable, but he will then have to explain why the sum of the two makes 0·5, and why the influence of heredity is less in the intellectual than it appears to be for the physical characters. There is the obvious direct scientific interpretation which seems to me the true one, environment does not act in one direction in *either* case, and the mental and physical characters are inherited precisely at the same rate. To those who have taken the trouble, as I have done, to examine carefully the mental characteristics of a family at intervals of a century apart, so that we are not troubled by the co-environment peculiar to brothers, it is needless, perhaps, to urge the very strong inheritance of mental qualities. If Prof. Cockerell attributes it to his third factor, "pre-existing soul," I should, indeed, be proud to have aided in the demonstration of its reality, although I fail entirely to see how it is to be done "by just such methods as Prof. Pearson employs." Meanwhile most people will, I think, prefer to stick to heredity.

Of the last paragraph of Prof. Cockerell's letter I understand not a word. Correlation is quite independent of variation, and although skull capacity is highly variable as compared with length of femur, I see no reason for supposing the former is therefore less strongly inherited than the latter.

I have not touched on the influence of "local races" on my data, because that appears to be a factor which has escaped Prof. Cockerell, and so I am not bound to state a doubt which I have well considered in order to reject it.

KARL PEARSON.

Magnetostriction of some Ferromagnetic Substances.

WE avail ourselves of your valuable Journal to give a short notice of some new results, obtained in our researches on magnetostriction, being a continuation of our investigation on the same subject, given in the "Rapports présentés au Congrès international de Physique," t. ii., by one of us.

It was generally believed that ferromagnetic bodies show change of length by magnetisation, but not of volume. Minute as the effect generally is, it is now placed beyond dispute that iron, nickel, cobalt and especially steel differ also in bulk in the magnetised state from that in the unmagnetised. In the course of our researches on the magnetostriction of different ferromagnetic bodies in the form of ovoids we came across a substance which shows a remarkably large increase of volume.

Examining the magnetostriction of reversible nickel-steels of different composition, which were kindly placed at our disposal by Dr. Guillaume and M. Dumas, we find that alloys containing 46 per cent., 36 per cent. and 29 per cent. of nickel all show increase of length several times greater than that observed in ordinary iron. But far more striking is the change of volume; of the three above-mentioned alloys, the effect increases as the percentage of nickel becomes less; with 29 per cent. alloy we observed a motion of 5 mm. of the liquid in the capillary tube (diam. 0·4 mm.) attached to the volumenometer containing the specimen (volume $v = 10$ c.cm.) under examination. Thus the greatest change of volume by magnetisation amounts to $\delta v/v = 51{\cdot}1 \times 10^{-6}$ in $H = 1690$ C.G.S. With ordinary iron, the same change $\delta v/v = 1{\cdot}2 \times 10^{-6}$ in the same field, being only about 1/40th of that observed in nickel-steel. The magnetisation of 25 per cent. nickel alloy is so feeble that it is impossible for it to be magnetometrically measured, and the change of length is inappreciably small, but the volume change is measurable and amounts to $0{\cdot}2 \times 10^{-6}$ in $H = 1790$. It is thus quite probable that there is an alloy containing somewhat more or less than 29 per cent. of nickel that indicates largest increase of volume by magnetisation.

We at first thought it would be possible to trace some connection between the thermal expansion and the change of length by magnetisation. No such relation seems to exist; the 36 per cent. alloy, which is the least expansible by heat, indicates tolerably large elongation by magnetisation.

As regards the Wiedemann effect, nickel-steels behave very much like iron, showing the maximum amount of torsion in moderate fields.

As is well known, the behaviour of cast cobalt, as regards the change of length by magnetisation, is opposite to that of iron, but the volume change in the same metal is much smaller. By annealing cobalt in a charcoal fire it assumes a pale ashy colour, and the magnetic character is greatly changed. The metal becomes less magnetisable, and shows constant decrease of length accompanied by increase of volume.

We have also found, by actual experiment, that the effect of stress on magnetisation and the magnetostriction in cobalt and in nickel-steel are reciprocally related to one another, as was already established for iron and nickel.

H. NAGASKA,
K. HONDA.

Physical Laboratory, Imperial University,
Tokyo, December 3, 1901.

Results of International Magnetic Observations made during the Total Solar Eclipse of May 17–18, 1901.[1]

To test further the results obtained by the United States Coast and Geodetic Survey magnetic parties during the total solar eclipse of May 28, 1900, regarding a slight magnetic effect that may be attributable directly to some change produced in the electrification of the upper atmospheric strata by the abstraction of the sun's rays due to the interposition of the moon between the sun and the earth, an appeal was made for international cooperation in magnetic and allied observations during the recent total solar eclipse.

The repetition of the observations was doubly interesting owing to the fact that the present eclipse occurred in the opposite magnetic hemisphere to that of the year 1900, and hence the opportunity was afforded for ascertaining whether the magnetic effect was reversed in its general character to that of 1900, as is, for example, the case with the diurnal variation in passing from one magnetic hemisphere to the other. The conditions, however, for obtaining observations at a number of stations distributed along the belt of totality, as was done in 1900, and thus testing whether the magnetic effect again followed directly in the wake of the shadow cone, were not favourable owing to the present location of the belt of totality.

In response to the appeal, simultaneous magnetic observations were made on May 17 from 14 to 21 o'clock Greenwich mean astronomical time—an interval amply covering the time of the eclipse—at a number of stations encircling the entire globe, three of which were in the belt of totality. The prime purpose of making the observations so as to cover the entire globe was to furnish the possibility of separating a possible eclipse magnetic effect from a contemporaneous magnetic storm of the usual type. The eclipse effect, for instance, doubtless would be confined to a very small belt, whereas a customary magnetic storm, in conformity with the usual experience, would manifest itself at practically the same moment of time over a very large area and thus be felt at stations far from the totality belt.

At none of the outside stations has a disturbance of any appreciable size been thus far reported to me, the general consensus of opinion of observers at these stations being that "nothing unusual occurred."

At the three stations within the belt of totality the majority of opinion is that something unusual did occur during the time of the eclipse.

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[1] Presented before the meeting of the Astronomical and Astrophysical Society at Washington, December 30, 1901.

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